

tainly it looks as if the influence of electricity in radioactive change, and its importance generally in its relation to matter, could be overestimated.

FREDERICK SODDY.

The University, Glasgow, July 29.

Stress in Magnetised Iron.

THE important question whether there is any mechanical stress in an iron rod or ring when magnetised, and, if so, whether the stress is compressive or tensile, was discussed in NATURE ten years ago (vol. liii., pp. 269, 316, 365, 462, 533), but has not yet, so far as I know, received any generally accepted answer. That a magnetised rod must necessarily be in the same condition as if under a mechanically applied compressive stress tending to shorten the iron, was, I believe, first suggested by myself (Phil. Trans., vol. clxxix., p. 216, 1888). Those who support this view generally speak of the stress as "Maxwell's stress," and assume its value to be $B^2/8\pi$. The stress in question seems, however, to be quite unconnected with the "stress in the medium" proposed by Maxwell, and its value is not in general exactly $B^2/8\pi$, but $(B^2 - H^2)/8\pi$. I have lately had occasion to consider the problem again, and perhaps I may be allowed to re-state my argument in a slightly altered form, and illustrate it by means of an imaginary model.

If a uniformly magnetised rod is divided transversely, and the cut faces are brought close together, the magnetic force inside the narrow gap will be $B = H + 4\pi I$. The force acting on the magnetism of one of the faces, and urging this face towards the other, will be less than B by $2\pi I$, the part of the total force due to the first face itself; hence the force per unit of area with which the faces would press against each other if in contact is $P = (B - 2\pi I)I = 2\pi I^2 + HI = (B^2 - H^2)/8\pi$. (In the case of an endless permanent magnet, $H = 0$, and $P = B^2/8\pi$.) The width of the gap may be diminished until it is no greater than the distance between two neighbouring molecules, when it will cease to be distinguishable; but, assuming the molecular theory of magnetism to be true, the above statement will still hold good for the intermolecular gap. The same pressure P will be exerted across any imaginary section of a magnetised rod, the stress being sustained by the intermolecular springs, whatever their physical nature may be, to which the elasticity of the metal is due. The whole of the rod, therefore, will be subject to a compressive longitudinal stress P , the resulting contraction, expressed as a fraction of the original length, being P/M , where M is Young's modulus for the metal.

Let a magnetic molecule of iron be represented by a rigid steel sphere, uniformly magnetised and covered with a closely fitting shell of india-rubber, to play the part of the "intermolecular springs." Imagine a straight row of these spheres in contact with one another, and kept in place by a force analogous to cohesion, which, while binding the spheres together, leaves them free to turn on their centres. This arrangement would, for present purposes, serve as a model of a filament of iron one molecule in diameter. If the magnetic axes of the spheres pointed indifferently in all directions, the attractions would be balanced by the repulsions, and the length of the filament would be the same as if the spheres were unmagnetised. If, however, the magnetic axis of every sphere pointed in the same direction along the filament, as would be the case when the filament was magnetised, the india-rubber between all the pairs of unlike poles would be compressed and the filament would be shortened. Let F be the compressive stress across the rubber between a single pair of poles, and s the amount, expressed as a fraction of a centimetre, by which the rubber is contracted; then, if there are n spheres, the total contraction will be ns (n being assumed so great that it is sensibly equal to $n \pm 1$), which is the same as would be caused by an equal compressive stress F applied at the two ends of the unmagnetised filament. The whole filament when magnetised may therefore be regarded as under compressive stress due to the magnetic forces, and since Young's modulus $M = Fl/ns$, where l is the length of the unmagnetised filament, the contraction expressed as a fraction of the length is, as

originally stated, F/M , the value of F in an actual piece of iron being $2\pi I^2 + HI$.

Sometimes there may presumably also be a longitudinal tension, as in the case of an iron rod placed along the lines of force in a uniform field, when the tension would be HI . In a ring electromagnet this would not exist.

As to what effect would be produced in magnetised iron by Maxwell's distribution of stress in the ether, I cannot venture an opinion. But if there is a tension, it can hardly have the familiar value $B^2/8\pi$, which is possible only when B is equal to H , and there is no magnetisation ("Electricity and Magnetism," § 643). My point is that an important component of the stress in magnetised iron is a compression which can be calculated and allowed for. The question whether or not this view is tenable is of the highest interest in connection with the possible correlation of magnetic phenomena, and urgently needs an answer.

SHELFORD BIDWELL.

The Mixed Transformation of Lagrange's Equations.

I SHOULD fancy from the review by "G. H. B." in NATURE of July 19 (p. 265) that the papers of Prof. Levi Civita relate largely to the mixed transformation of Lagrange's equations, the complete theory (Proc. Camb. Phil. Soc., vol. vi., p. 117; "Hydrodynamics," vol. i., p. 171) of which was first given by myself so far back as 1887. But what I wish to point out is this, that this theory depends no more on any so-called theory of "ignored" coordinates (or *kinosthenic* coordinates as Prof. J. J. Thomson [Phil. Trans., 1885, part ii.] calls them) than it does on the existence of the hypothetical personage known as the Man in the Moon.

The theory is merely the result of a piece of elimination, and is as follows:—Let the coordinates of a dynamical system be divided into two groups θ and χ ; let Θ and κ be the momenta of types θ and χ ; and let T be the Lagrangean expression for the kinetic energy. Then it can be shown that

$$T = \mathcal{T} + \mathfrak{K} \dots \dots \dots (1)$$

$$\frac{\partial T}{\partial \dot{\chi}} = \kappa \dots \dots \dots (2)$$

$$\frac{\partial T}{\partial \dot{\theta}} = \Theta = \frac{\partial \mathcal{T}}{\partial \dot{\theta}} + \bar{\Theta} \dots \dots \dots (3)$$

where \mathcal{T} is a homogeneous quadratic function of the velocities $\dot{\theta}$, \mathfrak{K} is a similar function of the momenta κ , and $\bar{\Theta}$ is a linear function of the κ 's.

By means of (2) all the velocities and accelerations of type χ can be eliminated from Lagrange's equations, and the result is expressed by means of the modified Lagrangean function

$$L = \mathcal{L} + \Sigma(\bar{\Theta}\dot{\theta}) - \mathfrak{K} - V \dots \dots \dots (5)$$

and

$$\dot{\chi} = \frac{\partial \mathfrak{K}}{\partial \kappa} - \Sigma\left(\dot{\theta} \frac{\partial \bar{\Theta}}{\partial \kappa}\right) \dots \dots \dots (6)$$

Equations (5) and (6) constitute the mixed transformation of Lagrange's equations, and include the equations of Hamilton as well as those of Lagrange.

When the coordinates χ are *kinosthenic* coordinates, that is to say, coordinates which enter into expression for the energy of the system only through their differential coefficients with respect to the time, all the κ 's are constants, and (5) is sufficient to determine the motion.

In § 173 of my "Hydrodynamics," the words "the latter of which does not enter into the expression for the energy of the system" should be omitted.

A. B. BASSET.

Two Modifications of the Quartz Wedge.

SOME little time ago I wished to make a quartz wedge for producing interference colours with the polarising microscope. The usual wedge supplied by optical instrument makers seldom gives colours lower than "clearer gray" of Newton's colour-scale according to Quincke, while the lower colours are often particularly valuable in petrological work. The quartz wedge is described in the

text-books on the subject (e.g. Rosenbusch's "Microscopical Physiography") as being cut "so that one of its faces is exactly parallel to the principal axis (optic axis, axis of least elasticity)." The difficulty in getting, say, iron-grey of the first order depends on the extreme thinness of the quartz required at the thin end of the wedge.

Now the interference colour given by plates of equal thickness of the same mineral depends on the direction in which they are cut, varying from a maximum when the plate is parallel to the optic axis to zero when the plate is perpendicular to that direction (assuming the mineral to be uniaxial). If, then, a wedge be made having one face parallel to some such direction as, say, an x or z face of the quartz crystal and its length in the direction of the trace of the vertical plane of symmetry through that face, it will give the same results as the ordinary quartz wedge, but, for the same thickness, will give a lower colour, so that the colours at its thin end may be got very low. On trial a wedge made in this way gave very satisfactory results.

The compound wedge described below, which, so far as I know, is also new, was found to be still better. Suppose a sheet of muscovite be taken, its axes of elasticity determined, and a strip cut of the same size and shape as the quartz wedge with the axis of greatest elasticity parallel to its greatest length. If the wedge is covered with the mica plate and examined between crossed Nicols, there will, of course, be a black compensation band in some position, and by cleaving the mica thinner this band can be made to move towards the thin end of the wedge, and finally to coincide with it. The mica is now cemented to the quartz, and a wedge is produced which gives all the colours of the first order. By the use of this compensation mica plate a very poor wedge may be converted into a first-class instrument, or one broken at its thin end restored to usefulness.

DANIEL JAMES MAHONY.

The Grand Hotel, Melbourne, Victoria, June 25.

Colour Phenomena in "*Boletus cœrulescens*."

ONE day recently in the woods at Lynton (where the soil is red) I found and gathered two very beautiful toadstools, with vermilion stem and bright, sulphur-coloured hymenium. In these individuals the striking colour phenomena peculiar to their family were remarkably in evidence; *in the brilliant sunlight* on the bright yellow under-surface of the pileus I found my name when traced in the most gentle way shine out almost immediately in the most magnificent of blues.

Will any of your readers kindly refer me to any recent papers concerning the chemical or physical processes which underlie this fascinating demonstration? From my own superficial observations it is evident, I think, that light plays an important part. The energy liberated by the very gentlest friction appears to be a sufficient initiative.

Parts that have been rendered blue, when left at rest, after a short time return to yellowness, but these same parts are capable under fresh stimulus, so long as the fungus is still alive, of again assuming temporary blueness.

The juice expressed from blue areas is itself bright blue, and imparts a bright blue stain to linen. Upon my handkerchief this colour remained so long (at least five hours) that I thought I had fixed it; but in the morning the dry blue patch of the night before was no longer blue, but yellow.

On cutting the stem its upper two-thirds was found endowed with the property of cœrulescence; but this was not in any degree possessed by its lower third, in which the cut surfaces remained of a reddish-brown colour. With the exception of the lower part of the stem and the cuticle, all the tissues of the fungus exhibited cœrulescence.

I take special interest in these observations on account of certain phenomena noticeable in human tissues in the course of a somewhat rarely met with pathological condition which has been described under the name chloroma.

Without entering into details, I may remark that along with the colour development which characterises this pathological condition hæmoglobin is probably being extensively

set free from red blood cells, and presumably this body or its derivatives are abnormally abundant in the body fluids. Is there any known organic iron-containing body capable of being responsible for these quick-change effects?

EDGAR TREVITHICK.

Strength of a Beetle.

LAST night a small beetle (*Aphodius fossor*), the length of which is $\frac{1}{2}$ inch, flew in at my window and alighted on a table next to me. As it buzzed about I put a lid of a tin box over it, but to my surprise the beetle walked about bearing the lid on its back. I then put the tin box on the top of the lid, and was absolutely amazed to find that the insect tilted up a corner of the combined box and lid, and nearly escaped. The weight of the beetle when dead was $\frac{1}{2}$ grain, alive I suppose it was a little more; but the box and lid weighed 1758 grains! Assuming that the living insect weighed 1 grain, it must have tilted up 1758 times its own weight! Of course, the strength required to tilt up a box on edge is nothing like so great as that required actually to lift the weight, but nevertheless the feat seems to me sufficiently astounding. The dimensions of the box are $3\frac{1}{8} \times 2\frac{1}{8} \times 1\frac{1}{2}$ inches.

CHARLES R. KEYSER.

The Gables, Hayward's Heath, July 26.

THE INTERNATIONAL CELEBRATION OF THE JUBILEE OF THE COAL-TAR INDUSTRY.

DURING the last century no discovery, perhaps, has led to such far-reaching and important developments as that of mauve, the first aniline dye, by William Henry Perkin. Not only was the door thrown open to the never-ending procession of artificial colouring matters, but the raw materials necessary for their production were also the raw materials for the synthesis of whole series of entirely different substances, which have now assumed most important positions in the world's daily requirements.

It cannot be too often repeated that Perkin's discovery was the result of true scientific devotion to pure research. The synthetic preparation of quinine was the goal aimed at—a sufficiently ambitious one for a lad of seventeen, for the problem is yet unsolved. Perkin did not state, as is perhaps too often done nowadays, that "only a black mass was obtained." His persevering and scientific habit of mind led him to investigate the "black mass," with the result that by extraction with alcohol was isolated the violet dye which is so closely associated with his name.

Great though Perkin's discovery was, yet greater still were the zeal, industry, and genius of the boy of eighteen which enabled him to make the dyestuff on the large scale and place it on the market successfully. Only those who have had experience in large-scale preparations can realise what this must have meant. New plant, new materials, new conditions: all had to be undertaken, and in the introduction of iron vessels for the manufacture of his raw material, aniline, Perkin laid the vast aniline oil industry under lasting obligation.

The start thus given, many entered the field; by a slight variation of Perkin's process Renard and Franc introduced the splendid crimson dye "magenta" in France, whilst shortly afterwards Simpson, Maule, and Nicholson started the manufacture of this colour in London. The happy collaboration of A. W. Hofmann, the college professor, with the splendid technical chemist and business man, E. C. Nicholson, soon not only placed the London firm in a commanding position, but gave to the world those researches on rosaniline for which Hofmann became so famous.